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taking partners occurs remains vague. How, moreover, in a world of objects experience of those objects can come in, where it comes from and goes to, is a mystery. This peculiar relation of togetherness-in-a-field, which is consciousness—how does it come to exist in a world of spatial and temporal relations? We have not space here to describe how critical realism explains these important facts; we shall merely assert that it has an explanation, and remind the "natural" realist that until he has one his theory is far from rounded out to such a dealing with the whole situation as shall command our attention.

According to critical realism, our fragmentary objective experiences are not parts of the really external world, they are pictures in our consciousness, or experience, of fragments of a homogeneous world which is really external to our experience and never comes within it at all. The conclusions of physical science are expressed in terms of our experience, but hold good symbolically of the world of things-in-themselves which forms the real environment of a human conscious experience. This doctrine meets the need, which "natural" realism can not meet, and which idealism does not try to meet, but which all men feel, of believing in a real, simple, homogeneous external world surrounding our narrow fields of experience. Whether the doctrine is true, and what the positive arguments are which support it, would be matter for another paper.

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THE FACULTY DOCTRINE, CORRELATION, AND EDUCATIONAL THEORY. II

V. THE METHODS OF SOLUTION

ALL doctrine which is to have pragmatic value, that is, which is to result in exhortations—the normatives rather than the normals of science—must issue in some definite practise which can be definitely tested. Unfortunately, the ordinary work of schools and colleges is so complex, and so many factors enter in as constituents to the results, that, though each side can claim the highest successes for its own doctrine, there is no evidence which opponents will accept. There is much valuable and interesting work already done by psychologists bearing on the issues previously mentioned; but they have mostly used units of work which educationists declare to be trivial and mechanical and therefore to have no or little bearing on the higher mental processes with which teachers are mainly concerned. It is argued further that to correlate the results of work in school subjects is useless, because no school subject is homogeneous mentally;

perception, memory, imagination, and reasoning are all involved and involved interdependently; and as one can not get one's units even approximately pure, the correlation of school subjects proves nothing scientifically. I ask psychologists if it would not be well to meet these objections by correlating certain mental processes within these school subjects themselves, using, doubtless, specially prepared exercises for the purpose, but still such exercises as may or can form part of ordinary school work.

First of all, then, we shall avail ourselves of the methods of correlation which are now readily at hand. If a mental process *A* is highly correlated with another mental process *B*, there is some presumption that a connection exists.

At the very lowest, such a relation enables us to infer from the quantified presence of the one to the quantified presence of the other; not indeed, inevitably, but with different degrees of probability. This is valuable knowledge both to the teacher and to the examiner; it will help the teacher in the classification of his children; and the examiner, if he knows the correlations, can cut down the number of separate tests which he will have to make.

But the teacher wants to know more, and he is apt to take the expression of a correlation as implying cause and effect. If some memory process, for example, were highly correlated with some process of imagination, he would suppose that, by training one, if it could be trained, he would be helping the pupil to improve in the other.

Is this reasoning justifiable? I fear not. Logically speaking, correlations are mostly between synchronous performances or attributes. But they *may* be sought for between antecedents and consequents, and if we can get them, however roughly, in the second form, we shall convince the educationist that experimental psychology has something to tell him of real importance to his work. The teacher is in search of the results of training in various functions, and of the results of the transfer of training, if any, from one power or faculty to another, from one subject of instruction to another. Correlations between coexistences are not all that is wanted; what is wanted also is direct experiment on the possibility of training and the possibility of transfer of improvement. And we must use tests which appeal to the educationist as having actual relationships to school work. What the teacher wants to know is the productive activity of his work, not merely the static relationships between the different parts of it. The early writers on correlation never overlooked the fact that correlations were not causes. Professor Karl Pearson in his "Grammar of Science" especially warns us that they are not. But there is a strong tendency, often realized, to regard correlation as implying more than a relationship of mathematical function. It is taken to mean a real relationship of mental functioning. Nor is it merely educational theorists who make this error. In Professor Hugo de Vries's "Plant Breeding,"[1] there is a significant sentence, especially as, in other paragraphs of the same chapter, he has clearly said that correlation may imply coincidence merely. The sentence runs, "In all such cases there can be no doubt that the coincidence is a real correlation and that the cause which darkens the flowers is the same as that which darkens the seeds." But the correlation is just as real in the case of the coincidences as in the case of causal correspondences. It is the vague notion that correlation *means* causal connection which gives rise to sentences like this.

What, then, can the establishment of correlation do for us? We wish, for example, to find the relations between two mental functions or two groups of functions. First, let us deal merely with their coexistence. To make my observations more precise and perhaps thereby clearer to educationists, let us suppose we set out to measure the relationship between the accuracy of numerical computation and accuracy in arithmetical reasoning.[2] Expressed in teachers' language, the question runs: Do those who work their sums most accurately numerically also solve arithmetical problems better than those who are inaccurate numerically, that is, in actual addition, subtraction, multiplication, and division? That these powers may be sometimes disjoined we know, if M. Poincaré's evidence be accepted. "Quant à moi," he says, "je suis obligé de l'avouer, je suis absolument incapable de faire une addition sans faute," and he speaks of Gauss as an exception among mathematicians, for he was both a brilliant mathematician and "un calculateur très précoce et très sûr."[3] And I have heard more than once in educational discussions that Newton could not work a simple addition sum correctly. The statement is usually an excuse for inaccuracy in arithmetic. The implied argument is that accuracy in numerical computation is not necessary to mathematical genius and, shall we say, the expectation that little Tommy, who can not get his sums right, may, nevertheless, some day, become a mathematical genius. The argument would be more to the point if it could be shown that Poincaré's eminent mathematicians and Newton himself could not do their sums *when they were boys.* For men of capacity tend to be bored by the very early stages of a study whose limits they are seeking to extend. From personal reasons I feel quite impartial, for I am neither a good mathematician nor a good calculator. But let us leave this question of adult psychology; it is not really germane to the issue. Back, then, to the children. We must measure the functions somehow and, if possible, without introducing artificial variants from the ordinary work of the child. Let us suppose we succeed in measuring one of them. How many measurements must we make? Suppose we make two or three, and endeavor to calculate from these a true value based on the conception that our measures are rather faulty, and that the variability in the measures is due to ''errors of observation.'' If we do, we are running counter to the opinion which all persons hold who are daily and hourly in contact with the work of school children. They insist that the children themselves are varying from day to day and that the variability is in the exercise of the function and not merely in our observations of it. And there is a further difficulty; the functions, unlike physical measurements, will not stand practically still while we are sizing them up. We can not measure a function without producing a change in it, mostly an improvement. And, unfortunately, all children do not show the same rate of change, not even those who start initially at the same level—if we may call the result of the first two or three exercises a level in any true sense. May we say that the first few exercises are a measure of initial ability or natural endowment, and that the later results are measures of general educability rather than of native capacity in the one definite direction? I incline to think that the facts of correlation between two functions under different conditions may appear to lead in that direction. But there are difficulties in the way. Consider half a dozen people just beginning to play lawn-tennis. Is their native capacity to play lawn-tennis shown in their first game, in their second game, in their third game, in their first dozen games, or after their *n*th game? I suppose, as a matter of common sense, we should say that their ''true form'' is attained—we are considering these six players only and their relations to one another—when, with equal practise, we could place them in definite order from which, for some time, they do not vary. Jones does not always play the same game, neither does Robinson, nor Smith, nor the rest. But Jones can always beat Robinson and Smith; and Robinson can always beat Smith, and so on. I choose an illustration from the problems of sport because, among Anglo-Saxon peoples, much more anxious thought has been bestowed upon such topics than upon mere workaday affairs. What does such a view imply, if accepted as satisfactory in relation to mental measurements? Some such condition as the following would result. We should need to keep on measuring our function until our list became practically steady. The steadiness required would not be a plateau-like regularity—I am speaking in curves—of *A*'s successive results. Such steadiness is very difficult to obtain with young people, though, in a few cases, by a careful arrangement of exercises as to difficulty and time, I have for certain purposes approximately obtained it. But indeed it is not needed to fulfil the condition required. We can rest satisfied that the true position of the pupil in our list of results has been attained when successive measurements do not cause any serious alteration in the relative position of the individuals tested. Expressed in mathematical language, the condition would be fulfilled when the coefficients of correlation between the successive results for the same group or list of children were highly positive. Let us, by and by, accept this position for a moment and see how it bears on much educational work on correlation—I am speaking, be it remembered, purely from an empirical standpoint, I make no claim whatever to deal with the mathematical theory of correlation. But before accepting it, let me illustrate the previous position by another modern instance.

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[1] 1907, p. 244.

[2] W. H. Winch, "Accuracy in School Children," *Journal of Educational Psychology,* December, 1910, pp. 557–590; and May, 1911, pp. 262–272.

[3] *Revue du mois,* July 10, 1908.

Is there such a thing as general suggestibility? If there is, it is sometimes supposed that if we measure the suggestibility in two functions of a number of individuals, the double series thus obtained will have a correlation highly positive. If there is no or little correlation, we conclude that there is no such thing as general suggestibility. Admitting for a moment that these inferences are valid, how shall we set about obtaining our data and our conclusions? Is it satisfactory to take one or two measurements, say, of suggestibility to temperature sensation, and one or two, say, of suggestibility to visual after-sensations (the so-called visual images), and correlate the two series of results? I do not wish to prejudge the question of the existence of general suggestibility; but I wish to give some empirical reasons for doubting the validity of the method.

Suppose I take ten tests of rote memory for meaningless things one day with a class of children (a class of children approximately of the same mental level and taught always by one teacher), and then, a few days afterwards, long enough for fatigue effects to have disappeared, I take ten other tests of exactly the same character. I shall probably find the positive correlation to be no higher than .4 or .5. Am I to conclude from that result that my tests are of different difficulty and that the difference in their "content" has produced this low correlation? It is a difficult supposition, for reasons which will be apparent presently.

A week later I give a third series which, perhaps, correlates a little more highly with the second than the second with the first. Still the correlation is very low between functionings which appear to us introspectively precisely the same. Can we account for the low correlation by errors of measurement? Hardly; the differences in many of the individual results for set 1, set 2, and set 3 appear much too large. Is it due to the functional variability of the children? Do they actually do different work on different days? We can readily suppose they might if we tested them in different conditions as to freshness and fatigue, if we repeated the tests at unequal intervals, or on different days of the school week, after different school exercises and different lessons. But if we are careful to avoid all these sources of low correlation, shall we still obtain the sort of results indicated above? I think we shall, in fact, with all the ostensible conditions equal. I have actually obtained the sort of results I have given above. But let us go on with the experiment: a fourth series is given, a fifth, a sixth, and more. Presently there comes a time in which each pupil settles down, so to speak, in his place, and the correlation between two of the later series may rise as high as +.9 (Pearson "r" coefficient) and with rank formulæ may rise to unity. This fact would seem to indicate that the initial variability was not due to the differing difficulty of the earlier series one from another, but to a true functional variability on the part of the children: also, that our methods of measurement were not very faulty, for, by a continuation of the same methods, we at last achieve some series of steady results.

We may now regard ourselves as possessing a reliable measure of the function for the period in question. And if we have in similar ways measured another function throughout the same period, we are in a position to find the correlation between them. Precisely what conclusions we may draw from the absence or presence of high correlation between series thus obtained is a question which, for the present, I leave on one side. I emphasize only the consideration that, from the series obtained from one or two exercises (especially those of a novel character) in each of two functions, we can not, at least with school children—I speak empirically only, and of the results with which I am personally acquainted—we can not, I say, hope to find a high positive correlation; for the earlier series, obtained by measurements of *the same function with the same "content,"* do not correlate very highly among themselves. For practical purposes we can obtain a correlation which will serve if we take the totals or averages of three or four sets of tests (given under the conditions referred to above) in each of two functions; but, strictly speaking, we may be fairly certain that our correlation is too low unless we have first obtained steady measurements in the functions themselves. Though then, with adults, there *may* be little of what can be called general suggestibility, still it does not seem that we are entitled to a negative conclusion without more certainty that the measures of the separate functions are reliable.

There is another aspect of the case which I may fairly be charged with having neglected up to the present, namely, specialization according to subject-matter. Let me illustrate the point by means of an example drawn from experimental work on memory. Suppose we are dealing with immediate rote memory for meaningless things: we set out to test whether the power to remember unassociated and meaningless figures is highly correlated with the power to remember unassociated and meaningless letters. But what precisely are we trying to find out? Are we expecting to find what we want by appealing to cases in adult life in which there may have been a long specialization preceding our experiments? Some of our adult subjects may have done a good deal of linguistic work and the memory of the juxtaposition of letters and words, at first, *bien entendu,* a meaningless juxtaposition with associations only of time and place, inevitably plays a part in all linguistic acquisition. Others may have worked much at a kind of mathematics in which memory for juxtaposed figures plays a considerable part, as, for example, the memory for roots, squares, and logarithms, not to speak of other memorized statistics required in most branches of applied mathematics and science. We can, of course, if it is thought justifiable, find out the correlation between *any* two series of results. We might get some notion of the influence of training if we knew beforehand the probable mnemonic values for persons of the same mental level without training. And that would indeed be valuable. But we usually suppose something rather different. We often tend to think implicitly that, when we have found correlations between two mental functions of a set of students, however variously trained, we have found out a value bearing directly on the truth or falsity of the faculty doctrine, and that we have discovered a value of directive service to pedagogy.

There are, I think, some considerations which make these opinions very doubtful. First of all, the faculty doctrine, except in a form which I attack rather than defend, does not require us to assert that, after various and diverse training, a set of students will be found in whom the positive correlations for these various rote memories for meaningless things are very high. The only form of the doctrine with which I am concerned asks about the relation of potentialities rather than of completed products. I do not for one moment intend to take refuge in mere potentiality. Like other experimenters, I measure potentiality by work done; but, as far as possible, I wish to avoid asserting potential relations when very different amounts of work have been done in the two functions which I am trying to correlate.

Let me put the matter more bluntly and from a pragmatic standpoint. The educationist wants to know whether there is not a kind of indeterminateness about this rote memory power. He wants to know whether, with equal training in both directions—the learning of letters and the learning of figures—there would not be found, usually, and on the average, a high positive correlation between the two functions. If we work with school children who have learned practically nothing at home, and measure only such children as have been exposed to similar educational environments in school, identity of curriculum, and of methods of instruction, we have a much better chance of discovering the relations of the potentialities. Calculating boys are hardly to the point, they are already specialized. We know that we can use up this memory power, for, by giving exercises hard enough and often enough, we can produce a condition in which, for a time, until fresh growth has occurred, we can obtain a descending series of results. Is it not reasonable to suppose that a long process of training in, say, the memorization of numbers will throw the mnemonic function out of balance and so give us results which, for a time at least in the young, and relatively permanently in those in whom the growth potentialities have been mostly realized and in whom growth is near its limit, will show high values for the results in the memory for figures and low values for the memory for letters? If this is the case, the correlation between the two series of results, when individuals differently trained are the subjects of the experiment, will not be very high even if the improvement of the one function produces some improvement of the other within the same mind.

There is a further condition necessary to success if we are trying to measure the correlation between two mental characters. Let me illustrate as before by means of the rote memory for figures and for letters. If we give so many letters for one test, we must give the same number of figures for one test. If we consider the accuracy or otherwise of the order of the figures, we must do the same with the letters. If we arrange the letters in rows of a certain number, we must do the same with the figures. If we allow a certain time for the memorizing of the letters, we must do the same with the figures. And we ought to measure individuals who have had no specialism in their training. Also we should measure the functions under equal conditions as to the subjects, freshness, and fatigue. If, for practical purposes, we are unable to fulfil these conditions, we must be prepared to accept our coefficient of correlation with the full consciousness that it is probably too low.

Suppose we make the functional conditions the same and are careful to take enough measures of each individual's work, we shall find, I venture to suggest, as far as school children are concerned, a number of rather high positive correlations between most of the qualities which we are accustomed to consider as intellectually good. Let me assume this result and ask what it implies.

VI. High Correlation and Functional Connection

May we infer functional connection from the presence of high positive correlation between two functions?

Shall we, from the general establishment of high correlation, find ourselves back again in the full flood of the faculty doctrine, nay, even more, back to a belief in the approximate unity of all mental function? The mathematical theory of correlation—I speak as a scribe or pharisee, accepting the authority of others—does not require us to go so far; and on empirical evidence, I should decline to go. Let us look at the logic of the question for a moment.

Two functional quantities α and β appear to vary together, and let us suppose that the establishment of high correlation of α and β comparatively for a number of different individuals has entitled us to assert that α and β vary together within the same mind. One function may cause another; they may both be dependent upon a common cause, or they may be merely coexistences. It is hard to believe the latter, though one school of psychologists and philosophers in the doctrine of psychophysical parallelism are pledged to it. If they depend upon a common cause, it may be one which is inaccessible to our operations. But our method would be to *try* to find it, operate upon it, and see if it produced these correlated variants α and β. Of course, if they were causally related we could operate on each and see if we could thereby produce variants in the other. It seems obvious that high correlation sets us a problem of connection. It does not *ipso facto* enable us to conclude that a relation of interdependence exists.

VII. Low Correlation and Functional Connection

Among educational psychologists, as I have said, it is frequently believed that a high correlation between two series of measurements in different individuals indicates that there is such a connection between the measured functions that growth in one is correlated in a highly positive way with growth in the other; and that improvement produced by training in the one is transferable to the other. Professor Thorndike, a long time ago (I think in the *Psychological Review*), pointed out to educationists and psychologists that these conclusions do not follow from the existence of positive correlation, and I have dealt with these problems in a preceding section.

But I now approach an issue in which I believe educational psychologists to be unanimously against the opinion which I shall hesitatingly suggest. I am going to advance the possibility of functional connection with low correlation. I speak always, be it remembered, of the mental powers of school children, and I speak always empirically. Suppose we measure a number of individuals in two functions, say, in rote memory for meaningless things and substance memory for stories.[4] We give three or four tests or more in each function, and find the correlation between the total results of the measurements of the two functions. With school children of nine, ten, and eleven years of age, this correlation is found to be decidedly low, perhaps scarcely twice as great as the "probable error." Yet practise in rote memory for meaningless things will, with these children, produce a decided improvement in their substance memory for stories. That is, there is a real connection between these functions though the correlation is small or doubtful. How can this be?

I make the following suggestion very tentatively. We are accustomed to measure the correlation between two functions *in the same mind* by seeing what relations of magnitude exist in functions α and β possessed by Tommy Brown, as compared with the magnitudes of the functions α and β possessed by Charley Smith and Johnny Robinson and the rest. Let us suppose that Brown remembers stories better than Smith or Robinson, but meaningless things badly, indeed worse than Smith or Robinson. Smith is intermediate between Brown and Robinson both in memory for stories and memory for meaningless things; and Robinson is best at meaningless things and worst at stories. Arranging in order (of course our cases are much too few in number except for illustrative purposes) we have:

	Rank in Memory for Stories	Rank in Memory for Meaningless Things
Brown	1st	3d
Smith	2d	2d
Robinson	3d	1st

There is obviously no positive correlation when we compare one individual with another. Must we therefore conclude that there is no correlation of the two functions within the same mind? Brown's capacity may be big in one direction and small in another *as compared with that of Smith and Robinson*, but an alteration in one of his functions may produce some alteration in the other. To find the connection of functions within the same mind, would it not be best to get a number of measures for the same individual and correlate these?

We can not, however, simply take a number of measurements of function α and then a number of measurements of function β in the same individual and correlate these without further consideration. For there is this further difficulty in the measurement of mental functions, especially in the young. The process by which the measurements are obtained will, almost certainly, produce a variation in the same direction in both functions. The second measurement of function α will give, generally speaking, a higher number than the first, the third higher than the second, and so on. And similarly for the successive measurements of function β. So that we may have produced a sort of correlation by our measurements. We may call this a correlation of improvability. Doubtless, some sort of common factor may be involved, even as vague a one as intensity of attention. How far intensity of attention can operate equally upon two different original endowments in two functions, one large and the other small, and how far it depends for its intensity of operation upon those endowments, are questions which I leave aside at present. We should, in young people at least, be almost certain to find two series of increasing numbers which would give a positive coefficient, even though the functions are not correlated when measured against those of other individuals. It may be interesting to the teacher to know that both functions improve with practise; he probably knew it before. But that is not really what he wants to know. He wants to find real connections directly between the two functions themselves. That is what he understands the faculty doctrine to mean. Perhaps these real connections may exist in the same mind even if there is low or doubtful correlation of the functions α and β when one individual is compared with others. And perhaps that real connection may not exist when positive correlation is found between two series of measured results of two functions α and β for the same individual. How then can teachers find out what they want to know? We seem at an *impasse.*

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[4] W. H. Winch, "The Transfer of Improvement in Memory in School Children," *British Journal of Psychology*, December, 1910.

Let me pursue the matter a little further. Suppose we take Tommy Brown and make a few measurements of function α and a few measurements of function β. If the results are fairly steady we may consider that the totals or averages of these results are a sufficiently accurate measure for our purposes of his capacities in functions α and β. Train function α by additional exercises; and let us suppose we produce an improvement upon his original work in function α. Now measure function β again. We shall be almost certain to find an improvement. May we set this down to the transfer of improvement from the training of α? Certainly not, unless we know how much is due to the natural growth of the function β, for Tommy is young, and is growing all the time. And how can we find that out? Pending the day when we shall have enough measurements under different conditions to give us approximate values for things of this kind, we must have "control" cases who have not been trained in function α to compare with Tommy; and this brings us to the method of "equal groups." By this method, one of two groups (of tested equality in function β) is trained in function α, the other is not trained. After a time both groups are again tested in function β. The excess improvement (if any) of the trained over the untrained group may be taken (subject to certain statistical conditions) as a measure of the transfer of improvement. We suppose that, during the period intervening between the preliminary and final tests of function β, the natural growth of the two groups in function β has been approximately equal, and this natural increase may account for a considerable portion of the improvement in both groups. It is, as I have said, only the excess improvement of one group over that of the other which we consider a transferred improvement.

Such a transfer of improvement means, if we have made our measurements properly, a direct relationship between functions α and β. They are related functions; improving α implies the improving of β; they form, in an intelligible and verifiable sense, part of the same faculty. The mental functions thus connected will, I believe, give "groupings" or "faculties" rather unlike those of early psychology.

VIII. A Modified Faculty Doctrine and its Relation to Pedagogy

Suppose that, finally, we are not entirely deprived of our faculties; suppose that the progress of experimental research, with more continuous tests and a clearer understanding of what formulæ of correlation can and can not do for us, restores us some of them, though perchance in a different form; what then?

Are we to plunge once more into all the errors consecrated by the old meaning of the term *Formale-Bildung?* Are we to believe, in English phraseology which was once very current, that the education of a scholar and a gentleman sufficeth for all things? Surely not. The answer we must give will depend on the quantitative relations of the improvements obtained by training related functions.

We can train memory? Yes, but to learn the things we need to know (I am speaking only of those things for which memorization is a suitable method) will train our memory as well as give us useful acquisitions. In order to give us justification for purely formal methods we should need to show that, by the formal training of function α, we can produce a transferred improvement in function β which we could not, with equal work, produce by training function β itself.

It is function β we want and not function α. If we can get more of β by working on α than by working on β directly, well and good; let us work for β by training α. I do not wish to deny that cases may exist in which this is possible, but they require experimental proof to determine them, and they are surely very rare cases.

In England, a few years ago, it was very customary, in elementary boys' and girls' schools, to train for drawing from the wrist by drawing first from the shoulder (free-arm drawing). The two functions were part of the same group of functions, they were part of the same educational faculty—the faculty of drawing. The procedure was quite in line with other inferences deduced from the faculty doctrine. No educationist thought it necessary to show that, with normal classes of senior children, the transferred improvement was greater than the improvement produced by a direct attack. Yet such a result is always demanded before the pedagogy of faculty-training is justified.

There is, however, another general consideration. Function β may not be accessible to direct attack. Well, if it is not, our problem is more difficult, but is solvable along the same lines. We must try our probably related functions, keep our records of improvements, and see which transfers most to β. But suppose β is practical efficiency in a trade or profession. Well, even then, we must try to get data of success or failure and compare with preceding educational practises. The results of such inquiries might, I fear, be painfully illuminating to many of us educationists; but if the balanced contentions advanced throughout this paper be sound, such a method is imperative if, in educational theory, we are to do more than oscillate violently between one extreme and another.

One cheering conclusion seems to me likely to issue as the outcome of work on the methods I have advocated. It may, I think, be found that education in the things we need to know for practical purposes, learning, imagining, and reasoning about them, will itself supply much of the training which a modified faculty doctrine would warrant.

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DISCUSSION

A PROTEST

DR. HOLLINGWORTH'S review of my "Text-book of Psychology" contains the following passage (this JOURNAL, Vol. VIII, p. 274):

> The two-level theory [of attention] is deliberately expounded in the text, although hidden away in the references for further reading is the confession that